conversion of a "stream" into a "tone," and "adding" to the "resonance" of such a converted stream, is very slipshod-writing. On the whole matter of resonance (p. 46) the writer is unsatisfactory. He does not include the cavities between the vocal ligaments and the lid (epiglottis) among the resonating chambers, except in the objectionable passage just cited, and he does not enter into the question of the modification of quality of tone by means of these resonances. By some accident in engraving Plate XIII. the letter W is placed on the windpipe, as well as on the cartilages of Wrisberg, and the vocal ligaments are not distinct enough. All the figures, XIII. to XVI., seem to be copied from the English edition of Madame E. Seiler's "Voice in Singing." It is a pity to waste space in such a little book on controversy. It was hardly necessary to quote Madame E. Seiler at length (pp. 81-90), and then controvert many of her statements. This only tends to confuse the learner. The result should be given from the author's own observations, and then, if desired, the points of difference might be explained in a note. Similarly for the controversy about the action of the "wedges" (cuneiform cartilages) on p. 45, which has no interest or use for a beginner. The space devoted to controverting Mr. Lunn's "Philosophy of Voice" (pp. 52, 69, 70), and to Mr. Illingworth's "hazelnut" theory of the "pockets," and other bits of controversy with Miss Sabilla Novello (p. 30) and Dr. Garrett (p. 32) might also have been saved with advantage.

It takes much space to point out a few minor blemishes that scarcely detract from the general merits of the book, which is clearly the result of much real work and careful observation.

OUR BOOK SHELF

Keith Johnston's Illustrations of Electricity and Magnetism. By W. Lees, M.A. (W. and A. K. Johnston, Edinburgh and London.)

MESSRS. W. and A. K. JOHNSTON have begun an excellent work in issuing these four sheets of diagrams in illustration of the fundamental experiments of electricity and magnetism. The subjects are well chosen, and with hardly any exception well drawn and coloured. They will be welcomed by teachers of science classes in schools for their clearness and general excellence. Mr. Lees, who has prepared them, has also issued a specially-written "Handbook" to accompany each sheet. Of these handbooks—though perhaps useful for such pupil-teachers as may have the misfortune to be set to teach a subject in which they have themselves never made a single experiment—the less said the better. The writer of them is in bondage to the ideas of half a century ago. Take as a specimen the following statement concerning the Leyden jar:—"Suppose, then, the accumulation of electricity in the jar to proceed, the quantity of free electricity in the inner coating goes on also increasing, *until the density of that electricity becomes the same as the density of the electricity of the prime conductor.*" The italics are the author's own! This is no more absurd, as a scientific statement, than it would be to say that when a dock-sluice is opened the water rushes in from the higher level until the muddiness of the water inside is as great as the muddiness of the water outside; for the electric equilibrium of two conductors no more depends upon the *density* of their respective charges than does the flow of water upon its degree of turbidity. Yet the writer of this amazing sentence styles himself "Lecturer on Natural Philosophy, Edinburgh." For the sheets of diagrams themselves we have nothing but praise.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

A Fourth State of Matter

MR. CROOKES has given us optical evidence of the existence of matter in a state of tenuity known hitherto only indirectly, and considers himself warranted in affirming the discovery of a fourth or ultra-gaseous condition; yet it can scarcely be conceded that he has demonstrated the truth of his views, or that his recent exposition of them has strengthened his position or satisfied the doubts of the sceptical. It is simply a question of the use or misuse of certain specific terms, and it is difficult to follow the logic which justifies the creation of a "fourth state" by the attribution of properties not differing essentially from those of matter in its normal condition. Before his contention be granted it should be proved that the substance under experiment possesses properties exclusively and inalienably its own; as rigidly defined as those which distinguish the solid from the liquid, or the latter from the gaseous.

By the abstraction from his experimental chamber of a large portion of its contents he has enlarged the interstitial spaces of the residual gas, and thus amplified the mean free path of molecular vibration from some millionths of an inch to several inches; but beyond this extension of the path of oscillation there seems nothing to warrant the opinion that the residual gas is essentially other than it was before.

If this amplification of the molecular path be the feature relied on for justifying the term "fourth state"—and this seems the only inference—then further travel in this direction brings us to a point easily within our conception, where the contents of the experimental chamber shall not exceed one or two molecules; and it becomes interesting to know if Mr. Crookes would then add a *fifth* to the other states of matter. To do so would seem the inexorable outcome of his reasoning, and inevitably resolves the question into one of the numerical contents of the chamber; and it rests with him to define the precise point where the ordinary conditions cease, and the *ultra-gaseous* commences.

In gases, whether at the normal density, or rarefied to 3 mm., we have an unbroken continuity of condition; which, contrasted with the solid and liquid forms of matter, is noticeable for the absence of any point whence a new state can be said to originate: would Mr. Crookes assign a vacuum of 0·999 mm. or one of 0·00003 mm. as the critical point in the attainment of his "fourth state" or some intermediate density?

Again, has Mr. Crookes fully recognised the distinction between the properties of matter *per se* and those which are referable to electrical agency as revealed by the experiments of Messrs. De La Rue and Müller, where the projection of molecules against the walls of the containing vessel is attributed to electrification; or, further, the fact that a tenuity approaching that attained in his experimental chambers has been long familiar to us in the case of steam of very high pressure?

Whatever may be the solution of our speculations regarding the ultimate condition of matter, opinion seems unanimous that the concrete form in which it is known to us consists of an aggregation of particles having immutable properties and composition, gaseous bodies being definite molecular groupings of such particles; and if such be the case, and the chemical character of the contents of Mr. Crookes' experimental chambers remained unaltered, it is difficult, if not impossible, to conceive the existence of any further condition other than that produced by the breaking up of the molecule into its component atoms.

London, July 9 GEO. E. NEWTON

Permanent Record of Foucault's Pendulum Experiment

SOME four years since, while arranging a Foucault's pendulum for use in the class-room, it occurred to me to endeavour to obtain a permanent record of the experiment, and as the results were very good, and the method simple, they may be interesting to others.

The pendulum used was sixteen feet long, the height of my lecture-room at the Massachusetts Institute of Technolgy, and

consisted of a cannon-ball weighing about 5 lbs., suspended by a fine steel wire, which at its upper end passed through a hole drilled in an iron plug. The pendulum would continue to vibrate for sixteen or eighteen hours after being set in motion. After obtaining satisfactory results by using a ring of sand in the ordinary manner, a very stiff bristle was attached to the terminal spindle, and under it was placed a thin smoked-glass plate. The resistance was too great to allow the bristle to strike against the plate at each vibration of the pendulum, so that the device was adopted of fixing the plate upon a heavy brass disk capable of being raised or lowered by levelling-screws. This was placed under the pendulum before the latter was set in vibration, and then carefully raised until the bristle scratched its trace on the smoked-glass plate. After two or three oscillations of the pendulum the plate was lowered, great care of course being taken to avoid all possibility of rotation during this operation. At the expiration of fifteen, thirty, or sixty minutes it was again raised, and this process was repeated as often as desired. The inclination of the tracings was beautifully shown, and its amount agreed exceedingly well with that given by theory. With a heavier ball and longer wire even better results might have been obtained, but the motion of the pendulum used was but very slightly interfered with by the friction of the bristle. I should not omit to mention that the details of the experiment were carried out by Mr. F. W. Very, then a student at the institute. CHAS. R. CROSS

Boston, Mass., June 19

The Freshwater Medusa

IN NATURE, vol. xxii. p. 218, Prof. Allman by mistake attributes to me the conclusion that Limnocodium has no marginal canal, and that its radial canals are not pervious. A reference to NATURE, vol. xxii. p. 147, will show that in my first publication on the subject I gave as a character of the new genus "Radiating canals 4, opening into the marginal canal. Marginal or ring canal voluminous." I made the same statement in my communication to the Royal Society on June 17, and have not since deviated from it. E. RAY LANKESTER

Artificial Diamonds

THE process of building up tubes, which Mr. Mallet has been so kind as to suggest to me through your valuable journal, has been tried, but was unsuccessful through the same defect as caused the failure of many of my other experiments, namely, leakage without bursting. Some of the tubes found empty would bear, when cold, a pressure of ten tons on the square inch without leaking, showing that the gases escaped through the porosity of the iron at a high temperature. Hydrogen and hydrocarbons seem to go through iron at a red-heat very easily, and the direction in which I am working is to obtain an impervious coating, or a method of "clogging" the iron, as seems to have sometimes taken place in the carbon experiments.

Experiments conducted since the reading of my paper have convinced me that the crystallisation of silica and alumina may yet be carried out with ease and certainty, and when I have rendered one of these processes a commercial success the experience gained in daily manufacturing operations will enable me to attack the carbon problem with much more certainty of obtaining definite results.

As I shall be writing an account of this work in the autumn I shall feel greatly indebted to any of your readers who, if they come across any not widely known experiments in this direction, will kindly communicate with me, so that I may have all the work done in this direction before me. Suggestions such as Mr. Mallet's are valuable to any worker, as the reactions of one brain must always be somewhat similar unless outside stimuli give new directions to its activity. I am always therefore thankful for either suggestion or corrections. J. B. HANNAY

Private Laboratory, Glasgow

Temperature of the Breath

THE average temperature of the interior of the human body, according to our best authorities, is 98°·6 F. What is the temperature of the breath? It might naturally be supposed that its temperature was the same as that of the interior of the body, or lower, if it is derived from the lungs, into which it is drawn from the cold outer air. But is this so?

The temperature of my body, as shown by the thermometer in the axilla and mouth, is normal, *i.e.*, about 98½°. On rising in the morning, before dressing or eating, I take the thermometer, wrap it up tightly in several folds of a silk handkerchief, and breathe upon it (expiring through the silk immediately over the bulb of the thermometer and inspiring by the nostrils). After five minutes of this operation I examine the thermometer, and find that it indicates a temperature of 106°·2. At 7 p.m., after brisk walking exercise, having eaten nothing since breakfast except a spoonful of boiled rice at 1 p.m., and having drunk nothing but half a tumbler of water and a monthful of ginger-beer, I take the temperature of my breath in the manner described, for five minutes. I find the thermometer indicates 107°. Again, immediately after dinner, at which only water was drunk, the thermometer shows my breath to have a temperature of 108°. At other times the thermometer will not rise under apparently the same conditions higher than 102° to 105°. A temperature of 109° was observed by the correspondent of an American journal, but he does not mention under what circumstances this occurred.

How is this high temperature produced? It cannot, as a friend suggested to me, be caused by the condensation of the moisture in the breath by the silk handkerchief, for if the temperature of the breath as it issues from the lungs be the same as that of the lungs themselves, *i.e.*, not exceeding 99°, the silk, soon becoming much hotter, would rather tend to volatilise than to condense the moisture of the breath. Is it caused by the friction of the breath upon the fibres of the handkerchief? I know of no observations to show that a high temperature would be so caused. Is it the actual temperature of the breath as it issues from the lungs? If so, then it is by the breath that the system gets rid of its superfluous caloric. For this elevated temperature is not communicated to the blood oxygenated in the lungs; the blood in the left ventricle of the heart (which receives this oxygenated blood) being, according to some physiologists, lower in temperature than the blood in the right ventricle, which has not yet entered the lungs.

The few experiments I have made seem to show that the temperature obtained as above described is higher when the surrounding air is warm than when it is cold. This looks as if more caloric passed off by the breath when less can escape from the general surface of the body.

How these high temperatures are produced in the lungs, if they are developed there, is a mystery. Perhaps some of your readers may be able to explain.

53, Montagu Square, May 27 R. E. DUDGEON

Reversals by Memory

I SHOULD much like to know if it be a common thing for people to *reverse* the positions of objects in the memory. An artist, on returning from the National Gallery, painted the *Téméraire* from memory. Taking his picture to compare it with Turner's, he found to his surprise that he had reversed the positions of the ship, tug, sun, &c. His daughter tells me that if she wants to refer to a passage in a book she as often looks for it on a left-hand page, while it is on a right-hand page, or *vice versâ*. Another lady, on looking at a wood-engraving made from a sketch which she had seen some time previously, asked if the engraver had not reversed everything? These are the only cases known to me.

Is the following universally true?—

Let some one write with a blunt instrument the letter P on your forehead, or anywhere on the front half of the head from ear to ear, and the P must be written backwards for *you* to "see" it correctly. But if it be written anywhere at the back of the head, it must be written correctly both for you and the writer to read it. The change takes place abruptly in a line over each ear. GEORGE HENSLOW

Toughened Glass

THE night before last a lady of my family emptied a paper powder composed of 7½ grains of carbonate of potash and 7½ grains of carbonate of soda into a tumbler of what is called *toughened glass* less than half full of cold water. After stirring the mixture she drank the contents, leaving a silver tea-spoon in the tumbler, and then placed the empty tumbler on the table by her side within perhaps a foot of a burning duplex lamp. About five minutes afterwards a sharp explosion occurred, which startled